VOL. 2. NO. 4.

THE Sidereal Messenger.

Conducted by Wm. W. PAYNE,

Director of Carleton College Observatory.

JUNE, 1883.

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To impress upon the mind the reality of the perfection of the works of the omnipotent, the living GOD.—Professor JAMES C. WATSON.

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The Sidereal Messenger.

"In the present small treatise I set forth some matters of interest to all observers of natural phenomena to look at and consider."—GALILEO, *Sidereus Nuncius*, 1610.

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On the Effect of Flexure of the Axis in Transit Instruments with the So-Called, 'Broken' Telescope. (By Prof. C. A. YOUNG, Princeton, N. J.)

This German form of instrument, with the eye-piece at the extremity of the axis, is much more convenient than the ordinary transit, and, if the reflecting prism is properly mounted, gives quite as accurate results.

In the ordinary form, however, flexure of the axis if symmetrical, does not affect the time of a star's meridian passage, while in the 'broken' transit instrument it does, and sometimes by a considerable amount. For instance, in the instruments used by the American transit-of-*Venus* parties in 1874 and 1882, it makes a difference at the zenith of nearly three-quarters of a second of time. Unless properly ascertained and allowed for it also renders it impossible to deduce the collimation error by reversal on a circumpolar star.

This flexure correction is not treated of in any of the common text-books on Practical Astronomy; certainly not in Loomis, Chauvenet, Brunnow, nor even in Sawitsch,

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who specially describes and discusses this peculiar form of instrument. The correction is, however, very easily applied, having the same coefficient with the level error, *and confounding itself in effect with the error due to the inequality of the pivots;* so that it may be completely allowed for by merely using $[b \pm (f \pm p)]$ for the level constant in place of $b \pm p$, (b denoting as usual the level error, obtained by direct observation with the spirit level, p the pivot correction, while f is the flexure constant.)

The theory is very simple: the weight of the instrument makes the axis sag in the middle, and this has the same effect upon the relation of the wires to the line of collimation, as raising the eye-end by the same amount. When the instrument is pointed to the zenith and the eye-piece is at the east end of the axis, this virtual lifting of the eye-end will cause a star to reach the wire, earlier than if there were no flexure, (and of course *vice versa* when the eye-end is west).

Now if we denote by f the amount of this flexure-acceleration when the instrument is set up at the earth's equator and directed to the zenith, then at any other latitude the effect upon a zenith star will be $f \sec \delta$, (since for such a star its declination, δ, must equal the latitude of the place). When the instrument is directed to a star whose zenith distance is z, then the whole flexure is no longer effective in accelerating the star's transit, but only that component of f which is perpendicular to the wires, i. e. $f \cos z$. The total effect upon such a star will be therefore $f \cos z \sec \delta$, and in this expression the coefficient of f is identical with that of the level coefficient in the ordinary (Mayer's) formula. It must be remembered, however, that f changes sign with reversal, being always + for eye east, and − for eye west.

There are several ways for determining this flexure constant f for a given instrument.

I. By observing zenith stars in reversed positions of the instrument. The star is observed on one or two of the outside wires with eye east; the instrument is then instantly reversed, and the star observed on the same wires with eye west.

The difference of the observations (corrected for level,

pivot error and collimation, and reduced to the middle wire) gives directly $2f \sec \delta$. But the collimation must have been determined by reversal on a horizontal collimator or meridian mark, and the hurry of the rapid reversal is unfavorable to accurate observation.

If the instrument will bear reversal without disturbance of azimuth, a more accurate result can be deduced from reversal on circumpolar stars above the pole, the collimation having been previously determined as above.

II. By means of the collimating eye-piece and mercury basin, or by a vertical collimator supported above the instrument.

The collimation must be determined as before by a horizontal collimator, and then observation of the image of the wires in the mercury basin, or of the vertical collimator, in reversed positions of the instrument will give f directly.

Professor HARKNESS employed this method (with the vertical collimator) in determining the constants for the instruments used by the transit-of-*Venus* parties.

III. When one has no collimators or meridian marks at his disposal, the constant can be deduced from a good series of star observations, not less than 20 stars; 10 in each position of the instrument, with circumpolars among them.

Calling a_e, the azimuth of the instrument for eye east, and a_w, for eye west, each observation in the first position of the instrument will give, when corrected for level and clock rate, an equation of the form $n_e = \triangle t + Aa_e + B(f + p) + Cc$: For the second position $N_w = \triangle t + Aa_w - B(f + p) - Cc$.

The solution of these equations by least squares will determine all five of the unknown quantities. If the instrument bears reversal without disturbance of course a_e and a_w become identical, and the unknowns reduce to four.

If a collimator (horizontal) or meridian mark is accessible, its observation will give the value of $c \pm (a_e - a_w)$, and we thus also reduce the unknowns to four.

If two collimators are accessible, one north and the other south, we can determine by them both c and $(a_e - a_w)$ and

thus reduce the unknowns to three, viz: Δt, a_e (or a_w) and $(f+p)$.

It is no disadvantage of this method that $(f+p)$ come out united, as they will always be used united.

The operation is somewhat laborious, but of course has to be performed but once for any given instrument.

It has been assumed so far that absolute flexure of the axis is constant in all positions of the telescope. Since however the central portion of the axis, instead of being cylindrical, is usually cubical, and moreover is pierced by the hole to which the telescope tube is attached, it may easily happen that the stiffness will vary sensibly in different positions of the telescope, especially if the axis is rather slender. In this case, terms involving other functions of z than the *cos.* will make their appearance.

Professor WATSON found that the observations with the Stackpole Transit, used by the Peking party in 1874 were considerably better repsesented by assuming a flexure correction of the form $(f \cos z + f^1 \cos^2 z) \sec \delta$, than by using the first term only.

In the case of an instrument in which the axis is as stiff as it ought to be the second term however is never sensible. In the Kahler Transit belonging to the School of Science Observatory at this place, the flexure constant is only 0."102, and I cannot find any trace of a term involving $\cos^2 z$.

It goes without saying that stiffness of the axis is much more important in this form of instrument, than in the ordinary form.

PRINCETON, April 1883.

PRIZE OF ROYAL DANISH SOCIETY OF SCIENCE.

The Royal Danish Society of Science recently offered its gold medal (£18 nearly) for a statistical investigation of the orbits of these small bodies considered as parts of a ring around the *Sun*. The form, position and relative distribution of mass, should, if possible, be stated with at least so much accuracy as is judged necessary for computing its perturbing influence on planets and comets—*Copermans*.

MOUNTAIN OBSERVATORIES.*

BY EDWARD C. PICKERING.

Much attention has recently been directed to the question whether the conditions are more favorable to astronomical observations on the summit of a lofty mountain than at the level of the sea. The evidence so far collected is somewhat contradictory, although there can be no doubt that for certain investigations great elevations are almost essential It therefore appears to be of interest to consider what advantages are to be expected, what work should be undertaken, and what instruments are best adapted to securing results of value. Astronomical science is at present limited not so much by the imperfections of our instruments as by meteorological conditions. The first cause of error is the change due to the effect of variations of temperature in the instruments themselves. This is especially noticeable in large reflecting telescopes, where a slight irregularity of temperature—such as would be produced by placing the hand upon the mirror—would entirely destroy the sharpness of the image. In any precise measures care must be taken that all portions of the instrument are at the same temperature, or serious deviations will ensue. The second and more important source of error is that due to the atmosphere. Owing to variations of temperature, the density, and consequently the index of refraction of the air, is constantly changing. This effect is magnified by a telescope, so that we always perceive the fluctuations in the images of a large telescope, such as are sometimes noticed by the unaided eye when looking through the column of heated air rising from a chimney. The effect of the absorbtion of the air is not very serious for objects near the zenith. At the level of the sea, about one quarter of the light of a star in the zenith is absorbed by the air. At the greatest elevations at which an observatory could be erected, only about one-half of the air would be surmounted. The brightness of zenith stars will therefore only be increased a little

* Read before Appalachian Mountain Club, March 14, 1883.

more than a tenth of a stellar magnitude,—an amount which is scarcely preceptible by the most careful comparisons. In observing the Sun, the intense light of the sky in its immediate vicinity often seriously interferes with the results. The observations of our fellow-member, Professor LANGLEY, and of Professor YOUNG, have conclusively shown the advantages, in such researches, of very elevated stations. The light surrounding a bright star or planet is largely due to the aberration, internal reflections, and imperfect transparency of the object-glass. It is not certain, therefore, that any great benefit would be gained by an increased altitude in observing the fainter satellites, or companions to bright stars. The principal advantage we should anticipate would therefore consist in the increased steadiness of the images. Should this result be realized, the importance of a mountain observatory would amply repay the inconvenience of conducting it. Not only would the accuracy of all measurements be increased, but close double-stars would be more easily separated, and the structure of the surfaces of the planets would be more distinctly shown. Stars too faint to be seen under ordinary conditions would become visible, owing to the concentration of their light in a single point.

We do not yet know whether more hours of good "seeing" can be obtained at a great height than at a properly selected point near the level of the sea. There can be no doubt that great advantages would accrue from a proper location; but it almost always happens that political or personal reasons determine the place where a large telescope is to be erected, independently of the best climatic conditions.

The difficulties of maintaining a large observatory at a great elevation are very serious. Among them may be named the expense of transportation of all the supplies needed, the unwillingness of observers to lead so isolated a life, its probable unhealthiness, and the difficulty of performing much work of any kind in a rarefied atmosphere.

The expenditure required for an observatory may be divided into the original cost of the buildings and instruments, and the current expenses for making the observations, reducing them, and publishing the results. If architectural

effect is not aimed at, the building expenses need not be large. The impression is prevalent that the principal expenditure should be made on the instruments. In establishing many observatories this has proved to be a fatal mistake. If much work is to be done, by far the largest appropriation should be made for current expenses, mainly for the salaries of a large corps of assistants. At the Harvard College Observatory, the current expenses for two years would cover the entire cost of the instruments. In other words, estimating the rate of interest at five per-cent, ten times as much is expended on the observations, reductions, publications, and other current expenses, as on the instruments. A similar remark applies to the observations at Greenwich and Washington. In most forms of routine work, such as is done at most of these observatories, the time requered to prepare the observations for printing may be estimated at five to ten times that required to make them. The relative expenditures would not be so great, since much of the reduction consists in copying and in simple computations, which can be done by less expensive assistants. Apart from the high cost of living on the summit of a mountain, high salaries must be paid to assistants, to induce them to make the sacrifices required in such a life. Obviously, then, great economy may be attained by restricting the work on the mountain to that which can be done there only, and conducting the greater portion at some lower elevation, where the best facilities exist for completing it. Among other advantages will be the convenience for supervision and direction of the work, rapidity in publication, and ready communication with other observatories.

The principal question to be determined appears to be whether the increased steadiness of the air will give a real advantage. For this purpose two similar instruments, which need not be of very large size, should be erected, one on the mountain, the other at some convenient point below. Similar work should be done with each for one or more years. The results would show the relative advantages of the two stations.

To proceed now to details. A form of telescope described

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in the Proceedings of the American Academy* has especial advantages for mountain observations. The telescope *A B*, Fig. 1, is mounted horizontally, pointing east or west, and has a plane mirror, *C*, inclined at an angle of 45° to its axis, placed in front of it. This mirror can turn around an axis coinciding with the axis of the telescope, so that any object when crossing the meridian can be brought into the field of view. The position of the mirror can be controlled and determined by long rods extending to the eye end. Objects can be observed only when within an hour or two of the meridian, but in any large piece of work there are always enough objects so situated. The usual method of erecting a telescope of the same size in a dome is shown for comparison in Fig. 2. Among the advantages of the horizontal mounting may be named the following. A much smaller portion of the rock surface need be prepared for its erection.

Fig. 1.

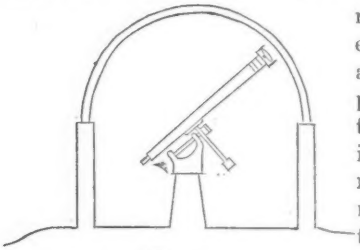
Fig. 2.

Only a small shed, which should be a part of the dwelling house of the observer, and a pier at *B C*, covered to protect the lens and mirror from the weather, are needed. The intermediate space below the tube may be left in its natural condition. A dome requires a prepared circular floor of a diameter somewhat greater than the length of the telescope. To derive the full benefit from a dome, the view should be unobstructed. The observatory must be on the summit of the mountain, where it is greatly exposed to storms, and where fuel and water are difficult to obtain. The horizontal telescope, on the other hand, only requires a clear southern horizon. It may, therefore, often be mounted where it will be protected from the most violent storms.

*Vol. xvi. p. 64.

The wind is a serious source of danger to a large dome. The figures show the relative surfaces exposed to it. Snow and ice are liable to render it impracticable to use a dome; while with a horizontal telescope this difficulty is reduced to a minimum, since it is only necessary that the shutter, over *C*, may be opened. A most important advantage of the horizontal telescope is the convenience to the observer. The consequence is that the hours of observation may be prolonged, and far more work accomplished. In a rarefied air, where every movement is fatiguing, the labor of moving a dome would be very great. With a horizontal telescope, as the eye-piece does not move, the observer sits in comfort, always looking horizontally. One of the greatest advantages is that the observer may work in a warmed room. The object-glass is so far distant, horizontally, that the heat at the eye-piece will not affect the definition. Work may thus be carried on with perfect comfort on the coldest nights. The application of this to observations during the prolonged nights in the arctic regions is obvious. Portability is often a serious consideration in work at great altitudes. No portion of the horizontal telescope need be so heavy but that it could be carried by a horse, or even by a man. The tube need only support its own weight, and may be made of tin. A heavy steel tube is required to give sufficient stiffness with an equatorial mounting. The horizontal telescope has especial advantages in steadiness, as mirror, object-lens, and eye-piece are all close to the supporting pier. This is an important consideration on windy nights.

Various researches suitable to a horizontal telescope have been sugested in the article named above. In general, the the greatest proportionate saving will be effected, when the least skill and time are required in the observations as compared with their reduction. Photography offers an especially promising field in this respect. The improvements in dry plates render it not improable that photographs will replace star-maps, for the brighter, and perhaps for the fainter, stars. A skillful photographer, even if he had but little knowledge of astronomy, might obtain in a short time a collection of photographs which, by a proper discussion in

a more convenient station, would yield a vast collection of valuable results. It cannot be long before a daily photograph of the solar protuberances, with occasional photographs of the solar corona, will form a part of the routine work to be expected from astronomers. The advantages of a great elevation in such work are undoubted.

The working hours of the assistants should be devoted almost entirely to observations. But little of the reductions should be attempted, as this can be much better done at the central office. A sufficient description of the work should of course be appended, to render the results intelligible to another person, but almost all the clerical work can be better done below.

Of course many meteorological observations could be taken with advantage in a mountain observatory, and by self registering instruments the time of the observer could be saved. The maximum force of the wind in storms, and the minimum temperature in winter, should be especially noted. An important investigation would be the measurement of the variations in the atmospheric refraction by the micrometer level (APPALACHIA. I. 63, 138), The apparent altitude of a number of the most distant points should be observed from time to time, and the atmospheric refraction deduced. Observations of the thermometer and barometer should be obtained at the same time. The value of the work would be greatly increased by simultaneous observations at one or more of the points observed. Such observations might at least be obtained at some point in the country below. Barometric observations at such a point, where the difference in height was as great as possible and the horizontal distance small, would have great value in determining the relation of the barometer to the height, as affected by other meteorological conditions.

In selecting a proper location, many preliminary observations would be necessary. We cannot depend altogether on the meteorological observations already made, as they commonly give the rainfall rather than the amount of cloudy weather. If a point could be found where the sky was almost always clear, at least in certain seasons, a great ad-

ditional advantage would be gained for many purposes. Much time is now spent in preparing to observe certain occasional phenomena, and no result obtained on account of clouds. With a certainty of clear weather, one observatory could often make all the observations needed of such phenomena.

The portability of the horizontal telescope is a strong argument in favor of its use in the preliminary observations. The first trial might be made with a telescope of the usual form, small enough to be carried by a horse. As high a magnifying power as it would bear should be employed, and images of bright stars, of close doubles, and of faint companions to bright stars should be studied as they approach the western horizon. By noting the time by which the image becomes unsatisfactory, we can compute the altitudes at which they are equally good on different nights. We have thus a quantitative test of the steadiness of the atmosphere in different places. The amount of air looked through is proportional to the secant of the zenith distance. Suppose that at a given location we could see a star to within 30° of the horizon, as well as we could see one in the zenith at the sea-level. Since the zenith distances in the two cases are 60° and 0°, and the secants 2 and 1, we should infer that the relative steadiness of the air at these places was as two to one. Repeating the observation with a number of stars, we should soon establish a habit of observing which would give a much better determination of the condition of the air than any ordinary estimate. The transparency of the air may also be well determined by the eye alone. It is only necessary to compare a bright star, while rising or setting, with others near the zenith apparently equal to it in brightness, and note the time of such comparison. A watch and star-map are all that are needed for these observations. The difference in the absorptions will then equal the true difference in brightness of the stars. The reduction may be made subsequently by computing the altitude of each star, and deducing the law of absorption by combining all the observations made at nearly the same altitude. These observations are so easily made that they

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are recommended to any traveller in a region where the atmosphere appears to be particularly clear.

So much time will inevitably be occupied by the preliminary observations, that some years must elapse before we can expect to find a mountain observatory in full operation. An important step would be taken by the Club if it could lessen this interval by aiding in the preliminary work. No mountain in this vicinity is sufficiently high to be used for such an observatory as is here contemplated. This objection would not, however, apply to the preliminary observations. Mt. Washington would not be suitable on account of the prevalence of clouds and the severity of the winds and storms. The same criticism may be made to the selection of Mt. Lafayette, Mt. Adams, or Mt. Jefferson. Their pointed summits, and the difficulty and expense of occupying these stations, also render them undesirable. Mt. Moosilauk on the other hand, although less elevated, has a large flat summit, well adapted to work of this kind. It is moreover within convenient reach of the railroad, and has a carriage road to the top, over which the instruments could easily be transported. The house on the summit would afford abundant accommodation for observers, and has already been occupied during the winter by a member of this Club. Probably no point could be found, having an equal elevation, where the preliminary observations could be conducted at less expense. Several boarding-houses at the base of the mountain would furnish additional accommodation, if needed, and greater comfort in case of accident or illness. Should any unforeseen difficulty present itself, nearly equal advantages would be possessed by Mt. Mansfield, the highest of the Green Mountains. A series of observations at one of these points would clearly show the difficulties to be anticipated at a more elevated station. For the latter it is doubtful if we could find a better point than Mt. Whitney, the station selected by Professor Langley. The geographical location and meteorological conditions of this mountain are particularly favorable. It is one of the highest summits in the United States, and is so situated as to be unusually free from haze, clouds, and storms. Al-

though at present somewhat difficult of access, it is probable that a railroad will soon be constructed to within a short distance of its base.

In conclusion, it is still uncertain whether any important advantage would be gained in most astronomical work by a great elevation, as compared with the best possible location at a moderate height. The question should, however, be decided, and it is believed that by the plan here proposed a sufficient test could be obtained at small expense. In any case valuable results would be attained, even if the observations in the observatories on the mountain summit were not much better than those at a less altitude.

DISTANCE AND DIRECTION OF THE CENTER OF OUR SIDEREAL SYSTEM.

JACOB ENINS.

After the discovery of the intergravitation between all the stars of our sidereal system, the conclusion is inevitable that they have a common center of gravity, around which they must all revolve. The conclusion also is inevitable, that this center of gravity must lie in the plane of the median line of the Galaxy, because the Galaxy includes nineteen-twentieths, of the stars of our system, and the remaining stars are about equally distributed on both its sides. This center must lie also in the center of that plane, because in a general way the stars are about equally numerous all around the Galactic ring. Fnally this center of gravity we must regard as the center of our sidereal system, the same as the sun is the center of our solar system.

We now advance to a new inquiry—Which way must we look for that center? The answer to this question is not difficult. We are nearly equally distant every way from the ring of the Galaxy, and if we were in the plane of the median line of that ring, we would be near that center. But we are not in that plane. This is proved by two concurring facts. First, the stars are more numerous on the side of the Galaxy toward its southern pole. This is because we are

situated on the north side of the Galactic plane, and when we look toward its south side, we see not only the stars on the south side, but many of those belonging to the north side as well. Second, the Galaxy approaches to within 27 degrees of the south pole, as marked on our celestial globes. But from the north pole it is distant 32 degrees. This difference of five degrees also indicates that we are on the north side of the Galaxy.

Therefore, being nearly equally distant from the ring of the Galaxy, and on its north side, we must look to find its center nearly toward the south Galactic pole. But all who visit our southern hemisphere observe, that in its southern regions the Galaxy appears a little brighter; which indicates that we are situated a little nearer to that side. Therefore, in looking for the center of the Galactic ring we must direct our search a little to the north of the south Galactic pole; which brings our view to about the constellation Cetus; more precisely to the tail of the Whale. Therefore, in that *direction*, we are compelled to locate the center of our sidereal system.

But the question next arises,—What is the distance to that sidereal center? Is it within the sphere of the stars of the first magnitude; or is it beyond the stars of the first magnitude, and between them and those of the second? Or is it between the second and third, or between the third and fourth; or is it, as may well be, far removed beyond the stars of the fourth magnitude? This is an inquiry of the very highest importance. In studying the mechanism of our sidereal system, and in learning the revolutions of its millions of stars around their central gravitating point, it is as important to know the distance to that point as it is to know the distance to the center of our solar system. We all know how much has been done to ascertain the distance to the sun;—how every transit of *Venus*, and how all other means have been seized upon to solve this problem. For similar reasons, *for more cogent reasons*, we must try to solve the problem of the distance to our sidereal center. Having found its direction, we must learn between which magnitude tars it is located.

In the present condition of our astronomical knowledge, the only means to discover the distance to our sidereal center, is, to find the angular distance between the median line of the Galaxy and a parallel great circle. This will appear as follows: It is estimated by astronomers that the distance to the Galaxy, or to its stars of the 9th magnitude, is so great as to require their light 2,000 or 2,500 years, to reach us. Here we have a triangle of which two sides and the included angle are known. The sides are the distance to the 9th magnitude stars of the Galaxy, requiring, say 2,000 years, for the passage of light. The included angle is the angular distance between the median line of the Galaxy and a parallel great circle. The remaining side will be expressed in the time for the passage of light through its length; and it will be the distance between the plane of the median line of the Galaxy and the plane of a parallel great circle; that is, approximately the distance between ourselves and the center of our sidereal system.

The problem now remaining is to find the precise angular distance between the median line of the Galaxy and a parallel great circle. This will be no easy task, and upon this nearly the whole question now depends. If we find the angular distance to be 2 degrees, then our sidereal center lies beyond the stars of the 4th magnitude. If we find $1\frac{1}{2}$ degrees, then it lies beyond the stars of the 3d magnitude. If 1 degree, then it lies betyond those of the 2d magnitude. If only $\frac{1}{2}$ degree, then it lies beyond those of the 1st magnitude. Various estimates have been made of this angular distance, ranging all the way from $\frac{1}{2}$ to $2\frac{1}{2}$ degrees, which estimates are of value to prove that we are on the north side of our sidereal center; but for determining our distance from that center, they are absolutely worthless. We must first ascertain where the median line of the Galaxy is. This will be a very difficult thing to settle, and will require much careful work. It demands the making of a map of the Galaxy on a very large scale—at least one foot for every ten degrees. This would make the map on an average about one foot wide and thirty-six feet long. Then we would be in a condition to draw this median line. I presume that its real median line would not

be straight, but wavy. This would have to be traced on the map. If it be wavy, nevertheless a straight line through it would have to be drawn, and this line would represent the plane in which the center of gravity must lie. Having decided on the position of this straight line, the next thing to mark on the map would be the parallel great circle. Then would come the measurement of the distance between them. The whole would require a great deal of careful judgment on locating the median line. The map should be constructed with the utmost care. Its lines of celestial latitude and longitude would nearly all be diagonal lines, and each one should be separately calculated. When correctly drawn and completely mapped, then could we estimate its waviness and the real positions of its median lines; both straight and curved. A small narrow map, such as are in common use, would not answer the purpose of finding the median lines. While undergoing study our large map would have to be laid on a table in a room more than 36 feet long. When not in use it could be rolled up. But an atlas of the whole should be made, consisting of twelve pages, each page three feet long and two broad. This extra width would allow the insertion of stars, important for their proper motions, and also of the many interesting resolvable nebulæ, near its borders.

This map and atlas would be valuable for the double purpose of defining the median line of the Galaxy, and of exhibiting the proper motions of its stars. On account of its large size, each well-ascertained proper motion could be shown by a diminutive arrow, the length of the arrow indicating the comparative velocity. By pursuing both these kinds of work, the proper motions of the stars, and the position of the median line, we have the best assurances that before long we would discover both the approximate distance to our sidereal center, and also which way the great ring of the Milky Way revolves.

It has been well said by a distinguished living astronomer that sidereal astronomy is to be the astronomy of the future.

Without any reference to our present theory, the vast ring of the Galaxy is by far the most important feature in

our sidereal system, and an accurate atlas of its face, to be improved with every succeeding generation, is one of the greatest of scientific desiderata.

It amounts to nothing to say, when we look at the proper motions of the stars, that there is no apparent center around which they all revolve. The same was long said about the very few members of our solar system, with their direct and their retrogade motions. Two great hindrances are in the way of our readily perceiving the center of our sidereal system. First, the fact that many hundreds of stars may have been turned by perturbation out of the plane of the Galaxy, and if so they may move at right angles, or nearly so, some one way and some another, around the sidereal center. Second, we are situated far on one side, away from the Galactic plane; and there being no great central sun, the stellar revolutions are in many ways obscured. For instance, we may look directly toward the sidereal center, and, seeing two stars near together, with proper motions, in opposite directions, we conclude the center cannot be there. But it may be there, for one of the stars may be on this side the sidereal center and the other on the other, and both revolving in the same direction and in the same plane. Or we may see two stars at equal distances from ourselves, one on each side the sidereal center, say thirty degrees apart, and moving in opposite directions. These also may be revolving in the same direction and in the same plane. Or we may look at two stars, one in opposition and one in conjunction, with reference to the sidereal center; they may move in the same direction at right angles to the sidereal center. Then, on a celestial globe, or in the sky, they would seem to move in opposite directions around the visible heavens; but really they would revolve in the same direction and in the same plane around the sidereal center. We may think of many cases where similar concurrent revolutions around the sidereal center would not be recognized from our not knowing the location of that center.

To discover the center of our sideral system by the study of the proper motions of the stars alone, has been found to be unsatisfactory. The theory of gravitation which in the past, has been so reliable in solving the greatest astronomical problems, will not fail to give the position of our sidereal center. It will show at the same time, that the Galaxy

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and all the bright stars, and all the unluminous bodies must revolve around that center. Astronomers believe that the rings of Saturn revolve. Why do they believe this? No one has seen them revolve. Why, then, this belief? Because of the force of gravity. They know that gravity would cause them to fall to Saturn if they did not revolve and thereby gain a centrifugal force to keep them in their orbits. For precisely the same reasons we must believe that the ring of the *Milky Way revolves.*

"*The American Ephemeris and Nautical Almanac*" contains much more matter than is found in the English *Nautical Almanac*, *The Connaissance Des Temps* and the Berlin *Jahrbuck*, and is regarded as a standard authority both here and in Europe. It is issued three years in advance of the time for which it is required. The volume for 1886 is now going through the press and will be ready for distribution in a few days. The computations are performed by the following corps of computers: Professors Van Vleck, Runkle and Keith; Messrs. Eastwood, Buchanan, Austin and Oliver, who reside in different parts of the country, and by Dr. Morrison and Messrs. Wiessner, Prentiss and Meier, Jr., who are in the office. The computations are all examined here by Dr. Morrison, who superintends the printing. The other gentlemen of the office, Lieutenants Vreeland and Stewart, Messrs. Loomis, Hill, Corrigan, Ritter and Meier, Sr., are engaged on the construction of new planetary tables. Besides the "American Ephemeris and Nautical Almanac," several other astronomical works of great value have been recently issued, consisting of a "Catalogue of Standard Stars," "On the Recurrence of Solar Eclipses," "Transformation of Hansen's Lunar Theory," "An Experimental Determination of the Velocity of Light," "On Gauss' Method of Computing Secular Perturbations," and a "Discussion of the Observations of the Transits of Mercury from 1677 to 1881." There is also in the course of preparation "The Development of the Perturbative Function of Planetary Motion in Powers of the Eccentricities and Inclination," by Professor Newcomb, assisted by Lieutenant Sturdy, Dr. Morrison and Mr. Corrigan. This development has been effected only by such distinguished astronomers and mathematicians as La Place, Hansen, Delaunay and one or two others. During Prof. Newcomb's absence in Europe, Lieutenant E. W. Sturdy, U. S. Navy, has been the acting superintendent under whose management the work of the office has been carried on with great efficiency.—*Army and Navy Register.*

THE COMETS OF 1882.

BY THE EDITOR.

The number of comets seen in 1882 was less than that of either of the two preceding years. In 1880 there were seven, 1881 eight, and in 1882 five.

The first of this list was Comet VIII, 1881 (Swift) which was visible and observed until the middle of January of 1882. The complete report of it belongs, however, to 1881.

Comet I. 1882, (Wells) is properly the first belonging to the year. It was discovered by Mr. C. S. WELLS, assistant at the Dudley observatory, Albany, N. Y., March 17. When first seen, it was a telescopic object with bright nucleus and narrow tail. After March 19, unusual interest attached to this comet, in view of the strange announcements made by Professor LEWIS BOSS, respecting its probable rapid growth. At that time its head was about 30″ in diameter, the length of tail, 6′ or 8′, and the nucleus was as bright as a star of the eighth magnitude. From rough computations, it was expected that the comet would rapidly increase in brightness and apparent size, and this expectation was fulfilled to the extent of its becoming visible to the naked-eye, early in the month of May. Those who computed its first orbit claimed that when the comet passed its nearest point to the *Sun*, it would be 6,000 times as bright as at the time of discovery. This value was somewhat diminished, by later and better computations but still great enough to give promise of a most interesting object. After perihelion passage, observers quite generally were disappointed in not seeing so brilliant a display as was expected, and yet the prediction that the comet would be seen in daylight was fulfilled, for it was so observed in several places, and notably June 10, by Dr. SCHMIDT of Athens, Greece, with a telescope of 6.7 inches aperture when it was only 2° 48′ from the *Sun's* limb. The reason why the comet seemed less brilliant than many supposed it would be, was, evidently, its low altitude and the twilight in which it was after perihelion passage. That it attained to the full measure of its brightness predicted for it by theory near the time

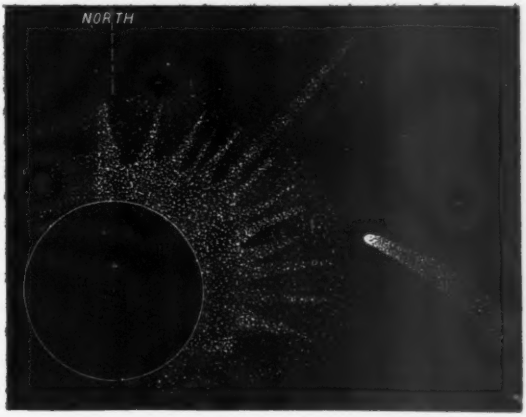
of discovery, there is scarcely any doubt. Indeed so good authority as Dr. MULLER of Potsdam, placed its brightness as determined by Zollner photometer at forty times that indicated by theory. The power of a deep atmosphere to quench the light of the comet was shown to be very great.

It should also be added that Professor Boss obtained meridian observations of the comet June 5.99, June 11 and 12, the last two being entirely satisfactory, and showing the comet as bright as an eighth magnitude star at night. The coma was round, sharply defined and three fourths of a second of arc in diameter. Observations were thus obtained within sixteen hours of the epoch of a rarely close approach to the *Sun*, the distance being not more than 5½ millions of miles as shown by several computors. These American observations are thought to be of value, in that they give a clear instance of the existence of a true nucleus.

The spectrum of this comet was important, because it was different from that of any other comet previously observed. All before had shown the characteristic bands of hydrocarbon, but in this, these were barely traceable in the continuous spectrum of the nucleus that was seen April 7. By April 21, the difference was strongly marked, and the spectrum was new and certainly peculiar to the comet. At the end of May the bright sodium line in and about the nucleus was seen at Dun Echt. In June, when the comet was brightest this line was observed to be double, the part most refracted indicating, according to Professor VOGEL, very great density of the incandescent gas. The shifting of the sodium lines, as compared with the Fraunhofer lines, was an evidence that the comet was moving in the line of sight at the rate of 3.7 miles per hour. The violet end of the spectrum was photographed by Dr. HUGGINS, requiring an exposure of seventy-five minutes, and the same difference marking the peculiar character of the comet was seen on the sensitive plate as shown in the visible spectrum. Dr. HUGGINS thought that the comet's original light as compared to its reflected solar light was much stronger than that of 1881, and that the continuous spectrum of the several bright places was due, possibly, to the sodium seen in the visible

spectrum. At least ten sets of elements for the comet's orbit have appeared, several of which show it to be elliptical but differing very little from the parabola.

The next comet was seen at Sohag, May 17, during the total eclipse of the *Sun*. Observers there photographed it on plates prepared to take the corona, as it was only about one-half of a degree from the *Sun's* limb. Its tail was thirty minutes long and its relation to the SUN well shown by the following drawing which is a copy from a photograph by Mr. A. C. RANYARD, of England.



This comet was never seen again, and probably will have no permanent place in our catalogues.

Mr. E. E. BARNARD, Nashville, Tenn., discovered a faint telescopic comet, Sept. 13. It was without nucleus or tail, and was traveling in a southeasterly direction in the constellation of *Gemini*. It was observed at Washington, Sept. 19, passed through the perihelion of its orbit Nov. 12, and was observed later at the Cape of Good Hope. It was several weeks in view of northern observatories, but parabolic elements of its orbit, only were computed so far as reported.

The fifth, and last comet of the year is commonly called the Great Comet of 1882. It has points of special interest still in discussion that deserve full and careful statement, and hence are reserved for a later issue.

(To be continued.)

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INTRA-MERCURIAL PLANETS—ONE, MANY OR NONE?

LOUIS SWIFT.

The late solar eclipse, the most remarkable astronomical event expected during the year 1883 has occurred, and the world is waiting with patient anxiety to learn the result of the two expeditions sent to the South Pacific to observe it. I have a personal interest in the success of the observers inasmuch as it will no doubt settle the controversy as to the existence of any circumsolar planets interior to the orbit of *Mercury*.

So much has been written regarding the supposed transits of dark bodies across the sun, that it seems unadvisable in an article as brief as this must be, to speak of them in detail. I only wish to say, that, having carefully examined the subject and weighed the evidence furnished by the observers themselves, I long ago arrived at the conclusion that no body of a planetary nature except those of *Mercury* and *Venus* has ever been seen on the *Sun's* face in transit.

While I contend for the existence of intra-Mercurial planets, I also consider them far too small to be seen on the sun's face under the most favorable circumstances and with highest powers.

Without waiting to hear what success the eclipse observers have met with on this point, I wish to again put on record, that, during totality of the solar eclipse of 1878 at Denver, Colorado, I saw two new stars which I have reason to think were intra-Mercurial planets, about 3° south-west of the *Sun*, some 6′ or 7′ apart, both ranging with the *Sun's* center, of an unusually red color, so much so as to attract immediate attention, and both of equal magnitude as to brightness and size of disks which were large for stars. They appeared fully as bright with my 4½-inch, power 25, field 1½°, as does the pole star in the clearest, darkest night to the naked eye. These are facts, and the world is challenged to disprove them. I had as fair a view of them three

different times as I ever did of a^1 and a^2 *Capricorni*. Though I swept over considerable space west of the *Sun*, these were the only objects seen. That there are no stars there answering the above description is conceded by all.

For a while I thought as did every one who investigated the matter, that one of them was θ *Cancri*, a star which Prof. WATSON says he saw, and the other a new star which he considered was an intra-Mercurial planet, (his planet *a*) discovered some 5^m previously.

A correspondence with him on the subject revealed the fact that while my two stars were equally bright, 6′ or 7′ apart, and ranged with the *Sun's* center, his on the contrary were 42′ a part, of unequal magnitude, and did *not* point towards the *Sun*.

Again, if we had observed the same objects and one was θ how could he, with a field ample for the purpose, have failed to see a star equally bright and but 6′ or 7′ distant? and how could I with a field of 90′, have failed to see his (planet *a*) only 42′ distant? When these facts were brought out, I came to the conclusion that we did not see the same objects, and, that instead of one, and probably two, there were three, and probably four discovered, there being a probability that his planet (*b*) was *Zeta Cancri*.

This subject attracted wide spread attention at the time, and will, I hope and sincerely believe, be revived when news from the parties reach us that the discoveries made five years ago by WATSON and myself have been verified, and that there exist planets nearer to the *Sun* than *Mercury*, and that though transiting the *Sun* often, are too small to be detected except during a total eclipse. I shall be disappointed to learn that Prof. HOLDEN and his party with their telescopes, and M. JANSSEN with his camera discovered none.

Warner Observatory, Rochester. N, Y., May 19, 1883.

A tribute to the eminent mathematician and astronomer, BENJAMIN PIERCE, LL. D., has been recently published by Moses King, Cambridge, Mass. Small quarto, pp. 64,

EDITORIAL NOTES.

The recent visit to America of the distinguished astronomer, OTTO STRUVE, was truly an important event to astronomy, and it is to be regretted that more could not have been made of it, in a public way, for the sake of the science generally in this country. This, however, was quite impossible, for his errand was one of business entirely. His stay, though short, received marked attention, and he was the honored guest of scientific circles in the East. Astronomers of the West coveted a like opportunity to extend a cordial greeting to one so well and favorably known in the development of modern astronomy.

As stated in an earlier notice, he came to test the great object-glass just completed for the Russian government, by Messrs. A. CLARK & SONS, Cambridgeport, Mass., and to superintend its transportation to Europe.

The livliest interest has been felt and manifested by prominent American astronomers, in the finish and performance of this great glass, both because it is the largest refracting objective in the world, and, also, because its size is at the limit of efficient working power as indicated by the theory of the mathematicians long ago. Observers, therefore, have waited almost impatiently, as shown by letters on our table, for the results of the crucial tests to which the glass would be subjected under the trained eye of the Russian astronomer. Of course no one can expect, at this early date, a complete official report of the examination; that must be made first to the Russian government. Enough, however is known of the tests used to give a fair idea of the excellence of the glass and the range of its power.

Professor STRUVE first went to Washington, and examined very carefully, with the 26-inch glass, *Jupiter*, *Sirius* and companion, *Procyon*, and distant companions, *Nebula* of *Orion*, Σ 3121, dist. 0.″4, period 39 years, 42 *Comae*, dist. 0.″5, period about 25 years. *Eta Geminorum* and companion, close double discovered by Mr. A. G. CLARK, a nubulous star, 450 of Herschel's catalogue, a peculiar object. The nebula is brighter on one side than on the other, and has a dark ring inside. He also tried nubula, Messier 59.

In testing his own glass at Cambridge, in a temporary mounting provided there for the purpose, he relied chiefly on the examination of 42 *Comae* and the nubulous star above mentioned. He was pleased with the performance of the glass on 42 *Comae*, for it gave two bright, round orbs of light with a clear-cut, dark channel, much better than he "had ever seen before." In looking at Herschel 450, he saw a second dark ring in the nubulous mass which he had not seen with the Washington glass. He was not sure that the 26-inch would not develop the second ring, for the 'seeing' was not the best while at Washington. The queer collection of bubbles near the center of this

great lens is not regarded as a serious defect at all. Professor STRUVE thinks that it will do no harm except to give very faint diffraction bands around a body as bright as *Jupiter*.

Professor STRUVE attended the April meeting of the National Academy of Sciences, at Washington, and gave an account of the examinations of his glass under the severe tests just named. He is thoroughly well pleased with the glass and highly compliments the makers.

THAT "HOLE IN THE HEAVEN."—On page 98, vol I. of the SIDEREAL MESSENGER, Professor Ormond Stone writes that, on occasion of his zone observations in −23° decl., he "found a remarkable vacuity in the region between $16^h\ 17^m$ and $16^h\ 25^m$ right ascension." There is nothing new in this; in fact, the absence of larger stars in that region was known about a hundred years ago to the elder Herschel. As it seems to have struck Sir William not less than Professor Stone, the following abstracts from the correspondence between Miss Caroline Herschel and her nephew, Sir John F. W. Herschel, bearing upon the subject, may be of interest. The letters are printed in "Memoir and Correspondence of Caroline Herschel, New York, Appleton, 1876."

Under date of Aug. 1, 1833, the venerable Miss Herschel writes to her nephew, who was then at the point of starting for the Cape of Good Hope, as follows: "Dear nephew, as soon as your instrument is "erected I wish you would see if there was not something remarkable "in the lower part of Scorpion to be found, for I remember your father "returned several nights and years to the same spot, but could not sat-"isfy himself about the uncommon appearance of that part of the "heavens. It was something more than a total absence of stars (I be-"lieve) But you will have seen by the register, that those lower parts "could only be marked *half swept*."

In a letter, dated Feldhausen, June 6, 1834, Sir John answers: "I "have not been unmindful of your hint about Scorpio; I am now rum-"maging the recesses of that constellation and find it full of beautiful "globular clusters. A few evenings ago I lighted on a strange nebula "of which here is a figure!" etc.

But the enthusiasm of Sir John about those nebulas and clusters Miss Caroline does not share, and apparently a little uneasy for having been misunderstood, she answers (Sept. 11, 1734): "I thank you for the as-"tronomical portion of your letter, and for your promise of future "accounts of uncommon objects. It is not *clusters of stars* I want "you to discover in the body of the scorpion (or thereabout), for that "does not answer my expectation, remembering having once heard your "father, after a long, awful silence, exclaim, "*Hier ist wahrhaftig ein "Loch im Himmel!*" and, as I said before, stopping afterwards at the "same spot, but leaving it unsatisfied, ..."

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Upon this then Sir John answers: "Feldhausen, C. G. H., Feb. 22 "1835, ...I have swept well over Scorpio, and have many entries "in my sweeping books of the kind you describe, viz: Blank space in the heavens, *without the smallest star.* For example:

R. A. 16h 15m	—N. P. D.	113° 56′	—a field without the smallest star.
16 19	"	116 3	—*Antares* (and Scorpio)
16 23	"	114 25 to 114 5	—fields entirely void of stars,
16 26	"	114 14	not a star 16 mag.—Nothing!
16 27	"	114 0	not a star as far as 114° 10′, and so on.

"Then come on the globular clusters, then more blank fields, then "suddenly the Milky Way comes on— ..... in large milky nebu- "lous irregular patches and blanks, with few stars of visible magnitude, "after a succession of blank fields and extremely rare stars above 18th "magnitude."

In the Cape observations ("results of Ast. Obs. at the Cape of Good Hope" page 381-2). Sir John Herschel gives more detailed statements where blanks occur by his southern sweeps, and he indicates not less than 49 of such places.

The astonishment of Sir William Hesrchel over that "hole in the heavens," alluded to in the above letter by his sister, is reported also in the beautiful sketch of the "Life and works of Sir W. H., by Edward S. Holden" (New York, Scribner, 1881). C. H. F. P.

May 9, 1883, Professor WILLIAM R. BROOKS, Phelps, N. Y., writes: "While engaged in comet-seeking on the evening of May 4, I observed a brilliant meteor rivaling *Jupiter* in brightness, and, by estimation twice his apparent size. It started in *Gemini* and disappeared near *Cassiopeia*, leaving a train of sparks lasting several seconds. The color of the fire-ball was at first a brilliant green, but it changed to a dull red before disappearance. It was the first meteor observed from this station during the present year.

Sirius, the leader of the starry host of heaven, so long in favorable view for study, is now out of reach of this hemisphere. Long ago BESSEL believed from the irregularity of its proper motion that this glorious star was a binary. In 1862, Mr. A. CLARK discovered its companion, a tenth magnitude star. Its position angle then was 84°.5; distance 10″.1 by BOND; 1865, SEECHI, p. a. 71°.4; dist. 10″.1. 1878 BURNHAM, p. a. 52°.4; dist. 10″.8. OTTO STCUVE thinks the mass of this companion must be half that of *Sirius*, but that its nature is different or it would shine as a first magnitude star.

Prominent American observers have given attention to this star during the year, and kindly furnish the following results, the first of which is communicated by Admiral SHUFELDT, superintendent of the Naval Observatory, Washington, D. C.:

OBSERVATIONS OF THE COMPANION OF SIRIUS.

Date.	Sid. Time.	*p.*	*s.*	*wt.*	Obs'r.
	h	°	″		
1883. Feb. 23.	6.5	44.0	10.02	2	F.
" 27.	6.8	40.6	10.01	3	F.
" 28.	7.1	40.9	9.39	3	F.
Mar. 1.	8.0	41.8	9.84	3	F.
" 7.	6.6	40.4	9.53	3	F.
" 8.	6.6	42.0	9.75	2	F.
" 9.	7.2	40.3	9.74	2	F.
" 14.	6.2	39.8	9.39	2	H.
" 16.	6.2	38.7	9.12	3	H.
" 17.	6.3	38.3	9.32	2	H.
" 18.	6.2	39.5	9.19	3	H.
" 20.	6.5	39.4	9.31	2	H.
" 21.	6.6	38.9	9.23	2	H.

Mean Results.			Observer.
	p.	*s.*	
1883.170	41°.43	9″.754	E. Frisby
1883.211	39 .10	9 .260	A. Hall.

DEARBORN OBSERVATORY, CHICAGO.—BY PROF. G. W. HOUGH.

Date.	*p.*	*s.*
	°	″
1882.991	41.9	8 98
1883.066	39.3	8.81
" .074	40.8	9.23
" .115	39.3	9.11
" .131	39.5	9.03
" .134	39.8	8.83
" .137	39.2	9.17
" .142	39.5	clouded.
" .159	39.7	9.02
" .175	39.6	8.99
" .195	38.0	9.03
Mean 1883.120	39.°7	9.″02

BY S. W. BURNHAM.

Date.	*p.*	*s.*
	°	″
1882.988	40.6	9.16
1883.000	40.3	9.27
" .074	41.1	8.99
" .107	39.9	9.02
" .115	39 8	9.17
" .134	40.1	8.90
" .137	38-1	9.17
" .151	41.0	8.92
" .153	40.6	8.91
" .159	39.6	8.86
Mean 1883.10	40.1	9.05

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CINCINNATI OBSERVATORY—H. C. WILSON, OBSERVER.

	H. A.	P. A.	Eye.	Position of Tangent screw	Power
Feb. 19,	+ 0.2^h	39.5°	Right, normal.	Forward.	450
	+ 0.2	42.3	Left, "	Backward	"
	+ 0.4	41.1	Right, parallel.	Forward,	"
	+ 0.4	42.4	Left, "	Backward.	"

Definition too poor for distance. Several false companions very difficult to distinguish from the true one. As I have not yet finished the determination of my personal equation I will simply take the mean of the four observations, which gives 1883.138, position-angle 41.32°.

OBSERVATIONS OF THE GREAT COMET OF 1882.

(Communicated by Vice Admiral Rowan Superintendent U. S. Naval Observatory.)

1883.	Washington Mean Time.	Δα	Δδ	No. of Comp's	Magnitude of Star.
Apr. 4	8^h 29^m 49.8^s	− 2^m 17.59^s	− 1′ 17.6″	12, 4	8

Comet α App.	Log. (ρ × Δ)	Comet δ App.	Log. (ρ × Δ)	Obsr.	Comp. Star.
5^h 57^m 20.58^s	9.5575	− 9° 18′ 27.5″	0.7877″	F.	W. 1449

Star	α	δ	Authority.
	1883.0.	1883.0.	
W 1449	5^h 59^m 37.14^s	−9° 16′ 53.4″	Bessel

Date.	(Obs.−Comp.) Δα	Δδ	Eph.
1825.	+ 4.06^s	+ 14″	Nature, No. 688, p. 227, Ast. Reg. No. 243, p. 72.

This observation was made with the 26-inch equatorial and compared with the following of three bright points in the nucleus. If we had compared the middle point of the nucleus with the comet, the corrections would have been:

$$\Delta\alpha = + 1.3^s \qquad \Delta\delta = + 0.3'$$

WASHINGTON, April 6, 1883. EDGAR FRISBY, Prof. Math. U. S. N

METEOR.—A few minutes past 8 o'clock on the evening of April 24 1883, a large meteor was seen by several observers in Bloomington, Indiana. It started in *Leo*, not far from *Regulus*, moved rather slowly, and exploded without detonation 45° or 50° above the northwestern horizon, at a point whose azimuth was 22° or 23°. According to Mr. J. W. WILEY the apparent magnitude was nearly one-fourth that of the full moon, and the intensity of its light such as to render small objects distinctly visible. D. K.

Professor J. ENNIS, Washington, D. C., contributes two articles, (the last in this number) calling attention to needed work in sidereal astronomy. He suggests the principles of universal gravitation as the basis of a theory for the motion of the sidereal system, and offers methods of work to obtain the necessary data to determine the direction of its motion. He thinks that the study of the proper motions of first, second and the greater magnitude stars only will avail little, because their distances from the Galactic plane are unknown, and that if their proper motions are, in all possible directions, that fact may be explained, when their relative positions are known.

But first of all, he claims that attention should be given to the proper motion of stars of the 6th magnitude, and under, that lie in, and near the *Galaxy*, and he thinks that these stars will show a proper motion in a common direction. He mentions Groombridge 1830, and 61 *Cygni* as notable examples of proper motion of the lower magnitudes. These stars have engaged the attention of astronomers not a little during the last few years; they have been thoroughly studied, and their proper motions are puzzling to all investigators because they are so great

There is no doubt but that the proper motion of small magnitude stars is highly important to sidereal astronomy, but the vast amount of labor involved in it, to secure accurate and general results useful in great questions like this has doubtless deterred competent men from undertaking it, especially when other work was at hand, demanding attention.

May 14, a message was received from Harvard college observatory announcing the discovery of a minor planet by Borrelly. Its position was, May $11^d.383$, Greenwich mean time, right assension $14^h\ 19^m\ 32^s$, decl. S. 14° 1′; motion in right assension -52^s; motion in decl. N. 7′; eleventh magnitude.

Mr. C. P. JACOBS, of Indianapolis, calls attention to a fine double star in *Lyrae* near star *Iota*. Its right assension is $19^h\ 14^m$ and decl. 35° 30′ N. magnitudes about 7½-8; 8-8. It does not seem to be noticed by either Webb or Proctor.

The Annals of Moscow observatory, (Russia), vol. IX, part I, contains an interesting memoir on Wells' comet, by Dr. BREDICHIN. Several drawings are given which correspond well with those made at the Cincinnati observatory. He concludes that the tail after perihelion was of his *second type* composed of varieties of hydrocarbons. Before perihelion the short tail seen contained heavier matter, which was proved by the spectroscopic observations of sodium. At the end of the memoir, he gives a review of the paper published by Professor SCHWEDOFF, of Odessa, last summer on "Les configurations de la comite, de 1882 *a* (Wells)." He criticises the paper severely, claiming

that Professor SCHWEDOFF is not well read concerning the structure of the tails of comets. He reproduces the figure given by SCHWEDOFF, and compares it with the drawing made by Mr. ELKIN at the Cape of Good Hope, and shows that the principal tail does not correspond at all to the prediction, and that neither this comet nor any other ever had an envelope tail such as is represented by SCHWEDOFF's desgin. H. C.W.

MR. WILLIAM DAWSON of Spiceland, Ind., saw the Great Comet of 1882 easily, as late as April 3. It had the appearance of a faint brush of light. Mr. DAWSON is preparing a catalogue of all the eclipses of this century. It was begun for his own use, because he know of no complete list. As the work went on he found a number of omissions from Chambers, and thinking that such a list would be of service to other amateurs he has wisely concluded to give attention to it and to complete it as soon as possible. In addition to the items found in Chambers he gives the magnitude of the eclipse, and the node at which it occurs. When the list is completed the MESSENGER will be favored with a copy.

METEOR IN DAY-TIME IN TELESCOPE.—On April 23, in the afternoon, wishing to observe the planet *Jupiter*, then about two hours past the meridian, I had set the circles and placing my hand on the tail-piece expecting to need a little movement in R. A., was very much surprised on looking into the eye-piece to see a brilliant, moving body. For an instant I scarcely comprehended the situation, but did so in time to follow. The meteor was moving comparatively slowly, and although sensibly diminishing in brightness, was kept in view until the zenith was reached and only then lost because the observer was on the wrong side of the iron pillar and using a prism. Estimated brilliancy at first sight equal to *Venus* at her best. G.

The following interesting observations from Baltimore appear late for want of space before:

February 5, 1883. At $7^h\ 50^m$ p. m. I commenced to observe *Jupiter* in order to see how long before egress, the III satellite could be discovered. The Nautical almanac gave $5^h\ 50^m$ the time of ingress and $8^h\ 31^m$ the time of egress. I immediately saw the satellite, but as a *dark disc* of about the same depth of shade as the darkest parts of the great belt. Its appearance was quite different from the inky blackness of the shadows in transit. I watched it until $8^h\ 5^m$ when I stopped to make records and rest my eye. At $8^h\ 15^m$ on resuming I could not find the satellite, but by $8^h\ 20^m$ could just discover a faint point of light and at $8^h\ 25^m$ the moon was seen as a disc of light brighter than the planet, and at $8^h\ 30^m$ it had reached the planet's limb, presenting he usual phase of egress. Definition was fair. While we have some

records of the dark transits of *Jupiter's* moons, I have not seen any accounts of the satellite showing itself darker than the planet and becoming brighter when at the edge. The whole time of this observation was 41 minutes. The time of transit was 2^h 41^m. The moon had completed three-fourths of its path when I first saw it.

J. E. H.

The French Academy of Sciences has awarded the Lalande prize to M. SOUILLART, for his investigations of the theory of the satellites of *Jupiter*. The Damviseau prize for the revision of the theory of the satillites of *Jupiter* was not granted; but an encouragment of two thousand francs was given to Dr. H. SCHUR for his recent determination of the mass of *Jupiter* and the eccentricities of the orbits of the first and second satillites." One of the two Valz prizes was awarded to Dr. HUGGINS for spectroscopic work, with favorable mention of the determination of the motions of stars in line of sight, and for photography of stellar spectra, nebula of *Orion* and the comets of 1881 and 1882. The other Valz prize was given to M. CRULS, director of the observatory at Rio Janeiro, South America.

Knowledge (April 6) says:

THE SIDEREAL MESSENGER, December, 1882 (Northfield, Minnesota). This little magazine contains chiefly borrowed articles and editorial notes. A paper from *Knowledge*, on Comet 1882, should not have been reprinted without some remark to the effect that the observations on which Mr. PROCTOR based his opinion respecting the speedy return of the comet were inexact.

"Chiefly borrowed articles!" *Knowledge* ought to know that such a statement is grossly untrue. As to Mr. PROCTOR'S prediction of the speedy return of the comet, we then thought, and still think that it was a unique piece of haste and bold guessing for a skillful mathematician. If Mr. PROCTOR had been less anxious to publish something definite about the period of the comet, when nothing certain could be known, at that time, from the best existing observations, possibly the 'little' MESSENGER would have been spared its first unpleasant criticism, which is now rather amusing, in that astronomers have not yet determined its period *certainly* after six months of exact observation and computation. It ought to be said, further, that the comet article was reprinted not at all on account of its sensational predictions (for reasons already stated), but rather because of a simple and really neat application of the principles of the Cycloidal Geometry, which we used with favorable comment before classes in the higher mathematics.

Mr. HENRY W. FAUST, of San Francisco, is more and more pleased with his new 6½-inch reflecting telescope (Brashear), as he continues work with it. His interest in the success of the MESSENGER is gratifying.

XUM

Professor HALL of the Naval observatory, Washington, D. C. has a brief but exeellent artiele in the *Analyst* for May, on "Kepler's Problem." In one paragraph, he sets out the nature of the problem admirably, and gives an insight into the working power of the indirect method of solution in the following language: "The indirect method, or solution by trial, is that, I think, to which every working astronomer finally comes. Of course, when the eccentricity is very small, or where tables of a planet have been computed, the equation of the center is tabulated, and this disposes of the matter once for all. But in case the eccentricity is as great as 0.2 or 0.3 and no tables are at hand, the indirect method does not require more than half the time of the other methods, and its accuracy is as great as one pleases. The advantage of this method consists in the indeterminate form in which the solution is left, and in the opportunity that is thus given for the intelligence and skill of the computor to come into use. Now this is the point that writers who are continually bringing forward solutions of the first kind (attempts to reduce the solution to a fixed set of rules) fail to see; and at the same time they seem to be shocked at the notion of making a good guess in solving an equation. But certainly it is not necessary to be stupid in order to be mathematical; and there are so many problems to be solved that we need not fear of taking an undue advantage of things, or that our ingenuity will ever grow dull for lack of opportunity."

Rev. R. M. LUTHER of Philadelphia, Pa., has been trying a 2.8-inch telescope with good results. We were recently favored with a considerable list of observations by the aid of this small glass which Mr. LUTHER himself mounted, that compare very favorably with standard work. He evidently has a good glass, and the careful way in which he mounted it, doubtless puts him in possession of its highest working power. April 19, he says: "For the benefit of such readers of the MESSENGER as have small telescopes only, allow me to say that last night I had most beautiful and distinct views of the colors of *Jupiter's* belts, by the aid of my little glass of 2.8-inches aperture. The brownish red of the southern belt, and the light green of the belts on the northern hemisphere were in beautiful contrast. The oval cloud-like markings of the equatorial belt were at times very plain. For about one hour the definition was very good, as may be judged from my seeing Ball's division on *Saturn's* ring. Let not the owners of small telescopes despair of making them useful."

Col. G. R. WEST, West's Hill, Dubuque, Ia. is desirous of purchasing a small telescope for his own use. He wishes a glass of superior quality or none at all.

Professor HENRY HARRISON, 35 Mandeville Anenue, Jersey City, N. J. has a small telescope for sale.

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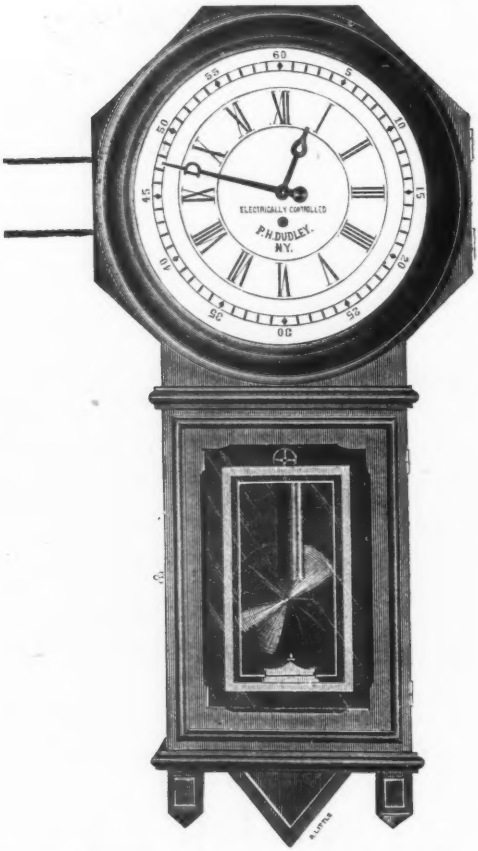

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